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DR. PAINE'S REPLY TO H. I. B.—No. IV.

DR. BOWDITCH states, that,—

"Dr. Paine never suffers his reader to lose sight of the *main object of his two volumes*, viz., a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) This is repeated, thus,—the numerical (or anatomical, according to Dr. P.) school."—(P. 74.) And yet again :—"It would seem, then, that our commentator raised up nothing but a spirit ; and we find him fighting as a *fundamental point* of the numerical method, a chimera of his own brain. This *two-fold error* of supposing the numerical and 'anatomical' schools *identical*, and that the *former trusts* to pathological anatomy as the ground-work of its system, *runs through* the whole of the hundred pages of criticisms."—(P. 79. *The Italics are mine.*)

I know not how to characterize these atrocious misrepresentations. I would most willingly avoid all offensive epithets ; and it is only the distinctness with which I shall show their desert that I can offer as a justification.

My work, in a general sense, aims at very different, and far higher objects. It is extensively concerned about nearly all the great subjects in physiology, pathology, and therapeutics ; and a *practical bearing* is everywhere given to the whole. Its general scope is to inquire into the prevailing philosophy in respect to each,—to indicate the methods of inquiry which should be observed by philosophers,—to exhibit the evils of hypothesis and the advantages of true theory,—to analyze our existing knowledge in the various branches examined,—and to wrest our facts from the grasp of speculation and reduce them to fundamental principles. Such are the objects of my work, and whether accomplished or not, it equally stamps the foregoing representations of my critic.

Let us now look at the statements more specifically. Dr. Bowditch presents me before the world, and before my work is known to the public, as having been guilty of the absurdity and injustice of "calling the numerical, the anatomical school," and of making them "*identical*." To enforce the intended effect, he also affirms that it is the "*main object of the two volumes*" to carry on "a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) Now, in the first place, I have devoted an essay, of 37 pages, to a consideration of the "Comparative Merits of the Hippocratic and Anatomical Schools." Here, then, if anywhere, this imputed confusion

should appear. There is not, however, in this Essay, a remote allusion to the numerical school. That school forms a special, but brief subject of consideration in the Essay which is devoted to the "Writings of M. Louis," and in my "Philosophy of Bloodletting." Morbid anatomy I have everywhere advocated, and have labored to point out its legitimate and important relations to pathology; but always holding it in subordination to the vital signs. The numerical school, in *M. Louis's* acceptance, I have condemned with as little ceremony as he has all preceding observation; nor have I any apprehensions of my success in this respect. It will be thus seen that Dr. B. has a comprehensive purpose in the foregoing statement. It represents me as deficient in common understanding,—my work as hostile to morbid anatomy as to "numericalism," and without that element in its foundation. A distinction is even cautiously maintained between the anatomical and numerical schools, which have no other than an incidental connection. At the beginning of my Essay on the Writings of M. Louis (p. 683), after saying that its principal object is "to exhibit 'the practical results of morbid anatomy,'" and that it "may therefore be considered so far a continuation of the last" (or the Essay on the Hippocratic and Anatomical Schools), I add, that, "we have also bestowed *some notice* in our first volume upon our author's *numerical system* in its relation to bloodletting; but, we shall *transiently notice it again in other aspects.*"

Again, however, there is no incompatibility between the two schools, whatever may be the abstract distinctions. The "numerical" may take the lead in the anatomical, and embarrass it with absurdities. But this only proves their elementary distinction. Bichat and Hunter were general anatomists of the highest consideration, each variously dissecting, but always rendering necroscopic results subordinate to vital phenomena. Therefore is it, that comparatively little of the former appear in their writings, especially those of Hunter. Their pages glow with nature as she glows, and they have but little of the odor in which the strict anatomist delights. The dross of the dead-house is subjected to an alembic, and a sweet distillation imparts its delightful perfume. Hence I have said in my work, that,—“Coming to Hunter, we find him analyzing the principle of life, and expounding the whole philosophy of inflammation, with scarce a reference to a post-mortem examination” (*Vol. 2, p. 674*); whilst, in another place I have it,—“who was neither *anatomist*, physiologist, surgeon, nor naturalist alone, but the most remarkable combination of *all these* that the world has yet seen.”—(*Ibid.*, p. 538.) All this is exactly true. Of Bichat, I have said,—“It is not, then, the *great architect* of the *anatomico-pathological* school, of which we are a *humble advocate*, who laid the foundation wrong; but the glare of his light was too much for his countrymen, who, as Armstrong expresses it, ‘have just burst from the old system of pathology,’ and who, as avowed by Travers, are unacquainted with the achievements of Hunter.”—(*Ibid.*, p. 663.) My opposition related to the “excesses of the dead-house” (p. 663), not to morbid anatomy as cultivated by Hunter and Bichat. But, morbid anatomists as they were, think you that either would have tolerated “*the numerical school*”? Andral and Louis are placed, by

common consent, at the head of the present anatomical school, whilst Broussais is ranked as a chief by a reviewer with whom I had the honor of an argument.—(Vol. 2, p. 645.) But, would Andral esteem it a compliment to be classed with the "numeralists," and did not my critic fear that the ghost of Broussais might call upon him for atonement?

Again, Dr. B. is pleased to say that "there is scarcely a hundred successive pages in either volume, in which this opposition does not manifest itself,"—that is to say, "a violent attack upon the numerical school,"—"the main object of the two volumes." Now there is no allusion to the subject, unless very incidentally (and of such I have no recollection), in any one of my Essays, excepting in those upon Bloodletting and the Writings of M. Louis. In the former, a short space only is devoted to its consideration, and I only recurred to it briefly in the latter. Here, in this Essay on the Writings of M. Louis, Dr. B. gives to his reader the *impression* that I have "dedicated a whole chapter of 134 pages" to exploding the "bigoted numeralists." Now it happens that I have "dedicated only three pages of this Essay to that object, and these are *isolated* from the rest by a black line drawn at their commencement and termination, that the individuality of this part of my subject might be at once obvious to the most careless observer. These three pages begin with the following sentence. Thus:—"Of the *numerical* method we have, perhaps, said as much as is incumbent upon us in our first volume, pages 293—309, 332. We have there exhibited an instance of its practical application in the hands of others (p. 305), and we will now present an illustration by our author."—(P. 780.) Indeed, Dr. B. has unwittingly indicated the amount of attention which I have given to the numerical school. Thus:—"Our author progresses in his zeal, and devotes *three* pages (Vol. 1, p. 293) to the improprieties of the numerical school" (p. 76), and just this amount, as I have shown, is given to the subject in my article upon the Writings of M. Louis. This Essay is mainly taken up in indicating the abuses of morbid anatomy,—in showing the generalizations of my author which he founded upon them,—in pointing out the dangers of false philosophy,—in arraigning my author for a departure from his own rules in philosophizing,—in protecting my brethren, from Hippocrates to the present time, against his almost universal scorn and derision,—in demonstrating the fallacy of the assumption that "medicine is now in its infancy."

The foregoing misrepresentation is also extended to the anatomical school. Now this school does not come under consideration in my first volume, excepting where I speak, in my Essay on Bloodletting, of the numerical method, and where, also, I bring forward morbid anatomy to *sustain* my argument with Dr. Hall as to the imputed effects of excessive bloodletting. And here, by the way, the few pages which I have

\* Pp. 73, 74. Dr. B. quotes these two last words with the manifest intention of imputing the epithet to me. Not having had the leisure to look over my work, I will not positively affirm that such an expression may not have escaped me. I believe, however, that the word "bigoted" is not in the book. The use of the word "strangled," in connection with bloodletting, is also imputed to me. (P. 75.) It is possible I may have committed the barbarism; but if so, I call upon Dr. B. to publish the remark, as well, also, that in which the words "bigoted numeralists" are said (or marks of quotation), to have occurred—the volume, and pages. If I am convicted, I fully allow the justice of the criticism. If I am not, \* \* \*

devoted to the *numerical* school are also isolated from the section by a line. The subject, too, as in the instances already cited, begins with a statement which makes a radical distinction between the numerical and anatomical schools. Thus:—"We shall now proceed to an examination of the treatment of simple pneumonia, and of some other inflammatory affections as founded upon the 'numerical method' in its connection with morbid anatomy as cultivated by the *exclusive* philosophers of the *anatomical* school."—(Vol. 1, p. 293.) It will be recollected, too, that Dr. B. refers to this very page, and had he fairly presented the subject in the foregoing acceptance, and called it *Louism*, I had not objected.

Coming to the second volume, I have nothing on the subject of the anatomical school in my *Essays on Animal Heat, the Philosophy of Digestion, and the Theories of Inflammation*; unless in the last indirectly, and not in the sense imputed by my reviewer. In my *Philosophy of Venous Congestion*, there is nothing upon the anatomical school, unless a casual reference, and here I bring up morbid anatomy, and far more extensively than has been allowed, as an *auxiliary* to my pathology of that disease. Finally, it is not, till near the end of the second volume, where my article on the Schools is placed, that the question is brought forward. In my *Philosophy of Venous Congestion*, a remark occurs which farther places this subject and my critic in their proper attitude. Thus:—"In our Essay upon the Comparative Merits of the Hippocratic and Anatomical Schools, and in our examination of the Writings of M. Louis, we have endeavored to show the superiority of the vital signs in marking the true pathology of disease."—(P. 316.)

Now, as to the second part of the "two-fold error," "that the *numerical* school trusts to pathological anatomy as the ground-work of its system." If M. Louis be assumed as constituting "the numerical school," then is the affirmation correct; otherwise it has no foundation. No one venerates more than myself the philosophical habits and labors of many (as Jackson, Hale, Gerhard, &c.) who employ the numerical method as an *auxiliary* to medical science. This remark leads me to say that I have been misapprehended in my views of the method, and perhaps I may not have been sufficiently explicit. Now it so happens that I like the method without its abuses, and as such have long employed it. Whenever I have spoken of it, I have always intended M. Louis's "numerical method," which rejects all observation that is not founded upon it, brings pathology and therapeutics under the dominion of mathematics, regards not the various considerations which relate to climate, constitution, habits, age, sex, &c., and practically knows little else than a *balance sheet*. As the method existed prior to this innovation, it offers important advantages as a general memorandum. It is of M. Louis's "method" and "numerical school," therefore, that I have spoken in my work. I would advocate the old method, or, as Dr. B. has it, "the plan originally proposed by others," and "we hereby give in our faith."—(P. 78.)

Dr. Bowditch brings me forward as the champion of Chomel. "Dr. Paine," he says, "seems to think himself called upon to defend the repu-

tation of Chomel," &c. Then follows a long exposition, implying that I have conveyed an impression that Chomel undervalues his friend Louis. The latter implication is without a shadow of foundation; and, as to the former, its only source is my remark that, "this distinguished observer, however, should be in no respect associated with our author's performance" (p. 685), and this, as will soon be seen, was modified immediately afterwards. Although I have now stated the whole of the "defence," Dr. B. nevertheless has it—"as our author thought it necessary to devote *two pages* to the subject, we were unwilling to pass it by unnoticed."—(P. 79. *My Italics*.) It is certainly painful to me to call up subjects of this nature, unless for the purpose of some general interests. Yet, being required to meet a calumniator, I must show the reader with what reference to truth I have been presented as the "defender" of Chomel. This will appear farther from the following extract from my work. Thus:—"And, *coming to the writings of M. Louis*, we shall make it a particular object to inquire how far both himself and Chomel are entitled to the rank which is awarded to them by the able writer who stands at the head of this Essay, as we have already endeavored to show with what consideration 'M. Andral is allowed by the profession to be the first physiologist in the world.'"—(Vol. 2, p. 645.) And now as to the charge of having placed Chomel in opposition to his friend Louis (p. 79), take the following passage, from my Essay:—"Not only such an example, but a multitude of them, occur in the writings of an author who will *not be suspected of any disposition to interfere with M. Louis's generalizations*. This author is Chomel, so largely interested in the works on Typhus, and Phthisis, now under consideration."—(P. 687.)

This last quotation carries us back to my 1st No., as it bears directly upon Dr. B.'s statement of my denial of Chomel's "superintendence" of the hospital. The remark occurs, also, within two pages of my general statement of the fact, as expressed in the "Advertisement," and of the misrepresentations which I am now investigating.

In reference to the controverted anatomical lesion of Peyer's glands, my commentator affirms, that,—“Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel;” and, to carry out this deception (and the memorable one connected with it, as shown in my second number), Dr. B. troubles the subject at great length. (P. 81.) Now, although I have introduced Chomel as the first of my witnesses because he was the *earliest* in order as to time, it will be seen by referring to my Essay that he is one of the *least* in importance; nor have I brought his testimony as having been arrayed (according to Dr. B.) against M. Louis's conclusions. On the contrary, it is immediately preceded by the last quotation from my work (p. 687). I have, also, even invalidated his statements with the “numerical school,” by saying that his “facts are rather *old*, but little known, and more frequently neglected.” I cannot, therefore, consent that my greater witnesses, Tweedie, Lombard, Perry, Craigie, Alison, The British and Foreign Medical Review, Dunglison, Geddings, Hale, &c., should be crowded aside for the benefit of the master. It is owing to this large

body of proof, clear and decisive, and the inferiority of Chomel's, that Dr. B. misrepresents the latter, that it may appear to follow that I have not aided in putting at rest a question upon which much of M. Louis's fame has rested. By turning to pages 688—692, the reader, who may have the curiosity, will see how the case stands between Dr. Bowditch and myself.

The perverted extracts which formed the subject of my second number are preceded by more than a page of introductory comment, which relates exclusively to the controverted anatomical lesion of Peyer's glands. The reader is here prepared for the coming event by imputing to me an unfair representation of Chomel's opinion of that lesion. This charge is conveyed in a manner so well introductory to a succession of deliberate acts of the very nature of the offence which they fabricate and condemn, that the reader will be gratified with seeing it. Here it is:—"The reader doubtless will suppose, from what we have extracted from Dr. P.'s remarks, that Chomel believes that the peculiar lesion of the intestinal follicles ascribed by Louis to the typhoid affection can be found in many diseases. Now we deny that Chomel ever said so, or meant to be understood so to say; and we assert that he declares exactly the contrary, and that it is Dr. Paine's *garbled quotation* that has led the reader into error."—(P. 82. *My Italics.*) I cannot encumber my reply with extended quotations from Chomel to show my translator's misrepresentation. But, as it was designed mainly as a *ruse* to give effect to the greater fraud which followed, I shall now state all that occurs, in my work, upon this subject in relation to Chomel, and of which Dr. B. predicates his affirmation that,—“Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel.”—Thus:—

“Chomel states that alterations of the glands of Peyer were common in the epidemic Parisian cholera of 1832; so common, indeed, that he was disposed to carry M. Louis's philosophy of the dependence of typhus upon the glandular affection to the cholera itself, and to establish an *affinity*, if not the identity, of these diseases. [*This is true.*] It is certain, that such had become the ascendancy of the glands of Peyer in ‘pathological anatomy,’ that, as in the typhoid affections, those glands were regarded by the scavans of Paris not only as the veritable *seat* of cholera, but as the cause of its morbid phenomena. [*And this is true.*] Thus:—‘On se rappelle que, dans l'épidémie de 1832, les premiers observateurs qui eurent l'occasion d'ouvrir des corps de cholériques crurent avoir trouvé dans la lésion des follicules intestinaux, et le *siège* de la maladie, et la *cause* de la plupart des *phénomènes* morbides qui la caractérisent.’ But, this is not all which Chomel supplies. He has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina, and other affections (sujets morts d'affections différentes); with the *exception of the ulceration*. [*And this is true.*] He states one case, in erysipelas of the face.”—(P. 688.) *This is also true*, and the end of my “ablest witness”! The reader will also not fail to ob-

serve, in the first part of the extract, how far I had placed Chomel in hostility to Louis.

The reader, by referring to Chomel's work, will see that there is no "garbled quotation," no words *fabricated*, or *expunged* from the quotation, and that my author's statement is fairly expressed. Nor should I neglect saying, that it was my principal object, in quoting Chomel, to show, through him, what had been the opinion of the "savans of Paris" as to the dependence of *Cholera* upon the follicular lesion. This is my great proof from Chomel, and it is undoubtedly important.

It will be seen, also, that I have stated the only exception of the least importance (that of "*the ulceration*"), as it respects Chomel's own cases, that the reader might judge for himself, whether the follicular lesion observed by Chomel in "other affections" than the typhoid, was not so nearly allied to that which had been observed by M. Louis in the latter disease, as to divest the typhoid affection of its imputed characteristic. This was my own opinion; but I did not say, as alleged by Dr. B., that it was the opinion of Chomel. I was merely stating a fact; and if any one thing, more than another, can show the trifling with morbid anatomy, and that it is made the basis of pathology by Louis, Chomel, and Dr. Bowditch, it is the very pottering about this lesion of Peyer's glands, and whether, as to Louis, at least, it shall be the *seat* and *source* of all the structural lesions and all the vital phenomena in the typhoid affection, when it may be *ulcerated*, or appertain to some other disease when it is *not ulcerated*, but having essentially all the other morbid attributes.—See B.'s attack, p. 82.

But, I have a few words more as to this charge of falsely representing the facts from Chomel. This is predicated of a fraudulent extract from my Essay. I shall place the right and the wrong ones in parallel columns.

*My Remark.*

"Chomel has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections différentes), *with the exception of the ulceration.*"

*Dr. Bowditch's Quotation.*

"Dr. Paine says that Chomel 'has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections différentes).'"

The reader will see that the important qualification is *expunged*; and having committed this act, Dr. B. proceeds *immediately* to say, "We are sorry to see such a lamentable deficiency in the fairness which we expect in one who *quotes*!"—(P. 82.) This was a fitting preliminary to the other mutilated quotation, and the false translation, which follow in immediate connection, and which were the subject of remark in my second number.

A fundamental anatomical question will begin my next number.



## HOMŒOPATHY, DR. TICKNOR, &amp;c.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—It appears that my statement, explanatory of the views of the late Dr. Ticknor, published in your Journal of Oct. 14th, is not entirely satisfactory to your correspondent H. He finds fault with the manner in which I treated the subject, and more than intimates that I had some ulterior object in view, besides that of defending the reputation of Dr. T. I confess, Mr. Editor, that in the present improved state of medical science, the result of two thousand years of experiment, research and discovery, I can look with little complacency upon any new system, or theory, which sweeps all this aside as of no value, as a mass of rubbish, from which scarcely any fragments can be gathered with which to build again. I have not so studied the ancients or the moderns, as to believe this can be done by any man, or generation of men, however great their powers, or acute their understandings, without manifest injustice—yea without exhibiting a high degree of arrogance, presumption and folly. But this Hahnemann has done—this his followers are now doing.\* Again, Sir, were this but a mere matter of speculation, not involving the lives and health of our fellow men, such trifling might afford no small entertainment; we might take up Hahnemann's works, as we do the Arabian Nights or one of Scott's novels, and knowing that we are dealing in the fictions of imagination, we might venture to give the reins to fancy, and for a while be absorbed in the musings and inventions of a speculative mind. But it assumes a different aspect, when we see our friends and acquaintances falling victims to this solemn jugglery—when we see life and health jeopardized upon the hazard of a hypothetical system, to the neglect of well-known, successful modes of treatment—and we may find our apology, perhaps, for warmth of language or severity of rebuke, in such considerations as these.

Your correspondent H. makes a belief in the infinitesimal doses essential to the character of a "*legitimate homœopathic practitioner*." Dr. T., then, was not a *homœopathist*; he was not even "*yet fairly in the right path*"—he had not even yet ventured "*on the only just course by which he could have come to a fair and honest conclusion*." In short, says H., "*he was not fairly inquiring into the validity of homœopathic practice, as taught by Hahnemann and his disciples*." This doubtless was the fact—Dr. Ticknor never did believe in homœopathy

\* Hahnemann denies that nature ever cures disease, or that Allopathic physicians can cure, except on the Homœopathic principle. He says (*Organon*, p. 148), "No severe symptom of a permanent disease has ever been treated by their opposite remedies and palliatives, where the evil did not re-appear after a few hours, more aggravated than before." "The miserable succor which the vital powers can procure, when abandoned to their own resources, is infinitely beyond the skill of the allopathist."—(P. 57.) "Until the present time, the diseases of mankind have not been treated according to a system founded on nature and experience; not by the remedies appropriate to them individually, but rather according to therapeutic notions, admitted upon the faith of mere imagination."—(P. 48.) "There is no true method (of cure) but the homœopathic."—(P. 145.) "Until the present time, no person has ever incanted this homœopathic mode of treatment, and yet more, no one has ever yet put it into practice."—(P. 49.) "How could the Old School, which was accounted rational, blindly take the vital power for its best instructor and guide; how could it venture, without reflection, to imitate the indirect and revolutionary acts which the vital power performs in disease, and finally follow it as the best and most perfect of models, whilst Reason, that magnificent gift of the Deity, has been granted to me, in order that we may go infinitely beyond it, in the aid which we are to bring to our fellow mortals."—(P. 35.)



as taught by Hahnemann—he never stultified himself, by instituting experiments with the infinitesimal doses, and it is presumed he never would. He certainly would not, unless, as I said before, he had lost his identity. There are some things, asserted as *facts*, which the common sense of mankind stamps, at once, as the fictions of imagination. There is no need of argument or experiment to determine their fallacy. The very fact of going into an examination of their merits, proves a man to be a weak-minded visionary, ready to believe in any impossibility or absurdity, if stoutly maintained. To set about, seriously, to establish the verity of Hahnemann's alleged facts, a man should be as visionary as Hahnemann himself. He should, like the poet, have "an imagination all compact"—ready, if he did not witness the desired results, to fancy he saw them. Dr. T. did not rank with this category. He did not believe in the efficacy of the decillionth of a grain of *charcoal, flint, lime, silex or sponge*!

I cannot here go into the discussion of the merits of homœopathy, nor is it necessary. H. finds fault with my assertion that Dr. T. "never maintained with homœopaths, that *the half is greater than the whole*," and says, "there is nothing in their doctrines which bears the resemblance to such, and this, Dr. Lee, if he *understands* anything of them, must know to be the truth." Well, perhaps I am ignorant, but I drew my facts from those whom I considered competent authority on this subject. I shall therefore have to bring them forward to testify in the matter. In the *Homœopathic Examiner* (*Vol. 1, No. 1, p. 5*) Dr. A. Gerald Hull remarks, "It (homœopathy) discovered that small doses of medicines affect the body much *more powerfully*, and also *much longer*, than the larger ones, which for the moment operate stronger, but are in general much sooner cast out of the system and lost." Again, Dr. Hull says (*Vol. 1, No. 2, p. 59*), "In consequence of the very slight diminution of power which many of the remedies undergo in the successive dilutions prescribed by Hahnemann, it has been affirmed by him, that the attrition by which the dilutions are effected discloses or develops, and even *increases their peculiar powers*." Again, "If by attenuation the surface of one grain of metallic gold be given to one hundredth part of a grain, it may affect the system as strongly as the whole grain."—(*P. 59*.) At page 83 of the same *Journal*, we are told by Dr. Ruchert, that *silex* "produces no dynamical effects unless homœopathically prepared and diluted, but when faithfully carried up to the 6th dilution, according to the directions of Hahnemann, and from thence up to the 30th" (which would require a mass of fluid larger than the whole solar system), "*it exhibits a great variety of potent symptoms*!" Dr. Hull's *Journal* is full of statements to the same effect.

If we turn to Hahnemann's *Organon* (translated by Stratten), p. 204, we read, "I have judged it more proper to administer only doses that are very weak and extenuated to a very high degree, because it is in this form that the virtues of medicinal agents are the most developed." Again (*p. 209*), "The more moderate the dose (without, however, going beyond a certain limit), *the more are the primitive effects developed* which are most important to be known." Again (*p. 295*), "Homœopathic

medicines acquire at each division or dilution, a new degree of power, by the rubbing or shaking they undergo, a means of developing the inherent virtues of medicines that was unknown till my time; and which is so energetic, that latterly I have been forced by experience to reduce the number of shakes to two, of which I formerly prescribed ten to each dilution"! Again (p. 298), "If the patient is very sensitive, and it is necessary to employ the smallest dose possible, and attain at the same time the most speedy results, it will be sufficient to let him smell once to a phial that contains a globule the size of a mustard seed, imbibing the medicinal liquid to a very high degree. After the patient has smelled to it, the phial is to be re-corked, which will thus serve for years, without its medicinal virtues being impaired." These are some of the passages I had in my mind, when I stated that Dr. T. "never maintained, with homœopaths, that the half is greater than the whole, and that the millionth of a grain of medicine had more power than the whole grain." I referred to the treatment of disease, and I stand ready to prove, if further proof be wanted, that this hypothesis pervades the entire Organon, and is generally believed and practised upon by homœopaths. Nothing is more common than to hear them say, that the high dilutions are more powerful than the lower. When I stated that homœopaths believed that "the billionth of a grain of medicine has more efficacy than a pound," I did not mean to be understood literally, but in a general sense. All I wished to convey was, that they hold that these high dilutions, or infinitesimal doses, were more effectual in the removal of disease, than the large allopathic doses, in ordinary use. I have proved that this is the case, and I leave it then to the reader to infer whose is the "gratuitous misrepresentation."

New York, Nov. 3d, 1840.

Respectfully yours,

CHARLES A. LEE, M.D.

#### CASE OF STAMMERING.

BY EDW. WARREN, M.D.

[Communicated for the Boston Medical and Surgical Journal.]

In consequence of some Remarks on Stammering, written by me several years since, for the American Journal of Medical Science, individuals afflicted with this difficulty have in several instances been referred to me by my medical friends in Boston. I have generally directed them to gentlemen in New York and elsewhere, who made the treatment of this affection their principal business.

In the course of the month of July last, however, a young lady was referred to me by Dr. Reynolds, who could not visit New York, but who expressed so strong a desire of relief, that I was induced to promise her such assistance as was in my power. She was afflicted with the worst form of stammering; that in which the difficulty proceeds from stoppage of the voice, and in which the organs of articulation become secondarily affected. On the other hand, the intelligence of the patient,

her ardent desire to obtain relief, and her never having undergone any course of treatment, were favorable to the prospect of cure.

My efforts were successful beyond my expectation. From the time of the first lesson she spoke freely, and subsequently no one would have perceived any defect in her speech, even when she was placed in the most trying situation, or in the utterance of the words she formerly found it nearly impossible to articulate. The continuance of the lessons for a short period was necessary to confirm the habit of easy speech; but when I last saw her she considered herself perfectly cured.

I stated in the "Remarks" above alluded to, that experience was absolutely necessary for the treatment of these impediments. For this reason, I have hitherto directed those who applied to me to those more accustomed to communicating the necessary instructions. On the other hand, I was encouraged to undertake the cure in this case, from the consideration that no person could have received more instruction upon the subject, or have seen more of the affection in his own person and in others, than myself. The pupil ought to become qualified in his turn to be teacher.

Most of the methods that have been adopted for the cure of stammering are empirical, and what happens to succeed in one case, will aggravate in another. We rarely see an adult stammerer in whom it has not been confirmed by the efforts made to relieve him. The affection, however, is now much better understood than formerly, and there is at least one method (for all the successful methods depend upon the same principle) by which a cure can in nearly all cases be accomplished, unless it has been confirmed as above said. In some cases, indeed, the pupil does not possess sufficient intelligence, nor will he make sufficient exertion to be cured. With an intelligent pupil a surprising and decided effect may be produced at once; but it requires great labor both upon the part of the instructor and of the pupil, in most cases, to render the benefit permanent.

Having been successful in this case, I shall feel myself bound to attend to such patients of this description as may think it a sufficient object to consult me at my present residence.

*Cambridge, Nov. 5th, 1840.*

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## BOSTON MEDICAL AND SURGICAL JOURNAL.

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BOSTON, NOVEMBER 18, 1840.

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### CORONERS.

ENOUGH has been said in the English journals, from time to time, to convince any community of the propriety of appointing medical men to the office of coroner. The topic has been often discussed in the United States, but the old system still prevails, and therefore it not unfrequently happens, in some places, that the coroner is a person neither respected for his intelligence, acquirements, or moral standing. If it is true that physi-

cians are the best qualified for conducting examinations over a dead body—that they must ultimately decide upon the causes of death under suspicious circumstances, and that in cases of wounds, contusions, &c., they influence an inquest, then it is obvious that they would be the best men for coroners. There is often a useless waste of public money, and it may be an unnecessary excitement may sometimes exist, in consequence of the ignorance, stupidity or cupidity of incompetent coroners; whereas, had a physician the entire control, he would, in many such cases, at once understand the character of the matter, and save all that hurly-burly and confusion that grows out of an excited imagination, when the cause of death is not distinctly known in the neighborhood where it may have occurred.

The important responsibilities appertaining to the office of coroner, positively require more elevated qualifications than are ordinarily found in individuals holding that trust in New England. This is admitted, even by the lawyers, who are certainly conversant with the unscientific manner in which coroners' juries are very generally conducted. Innocent persons are frequently, we apprehend, imprisoned and tried for their lives, because there was a lack of that sort of knowledge in conducting an inquest, which is expected in a civilized community. Were medical practitioners generally appointed—a courtesy which has not yet been extended to them by any executive hereabouts—the change would certainly redound to the public advantage—and it would no longer be viewed as a kind of degradation to be a coroner.

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*Purchasing a Practice.*—Gentlemen in pursuit of a location to practise, are becoming a numerous body. It being quite impossible to find an unoccupied place, the prominent object of solicitude is to ascertain who is in want of a partner, or who is desirous of relinquishing business. It has been found, by experience, that the shortest way of doing this, is to speak through the medical journals. A single line, in the form of an advertisement, will accomplish more for the advertiser in one week, than a whole year devoted to travel in search of a promising residence—and hence the custom of advertising by those wishing to dispose of their practice, as well as those who wish to procure one, is becoming very general. In London there is a class of medical brokers, exclusively devoted to negotiating between persons of this description. The same custom may at some future time prevail here. As a general rule, an objection is made to purchasing the house, fixtures, and the like, which the seller may happen to possess, and which would be as useful to the purchaser as they were to the owner, in pursuing the same professional routine. Again—in most instances, there is an unwillingness to pay a bonus for the good will or patronage of the occupant. Both of these objections are generally wrong. If a stranger can step at once into the custom of a physician, who perhaps may have been twenty years in obtaining the confidence of the community, it is right and proper that he should pay for it; it is economy to do it, since he is thus introduced to the patrons of the seller, whose ability to put him into the immediate receipt of a substantial income, it is presumed has been first ascertained. There are at this office several letters, from persons who have advertised in the Journal, who from ill health, or other causes, would willingly part with their practice for a reasonable compensation; but without some indemnity for transferring it, they would be wronging their families to do so. Some have small estates, in a majority

of instances worth the price asked; but in all transactions of the kind, judicious appraisers should fix upon the true value of the property. The risk, therefore, does not amount to an obstacle in the way of a fair trade. We are quite confident, having watched the progress of it some considerable time, that young physicians would be gainers, as a general rule, by purchasing the stand of a practitioner of character. It is not difficult to ascertain every fact that it may be essential to know—and the cost should be proportioned to the extent of the field. Reference is here made to the country, exclusively. To purchase a practice in a city would be ridiculous: a transfer could not be effected, constituted as society is and always must be in a dense population. In the country it is reasonable and judicious to buy and sell a medical patronage, and the expediency of the measure will ultimately, we believe, be universally admitted.

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*Dr. J. Dunglison's Report.*—A pamphlet of thirty-five pages, octavo, has been received, entitled "a second appeal to the people of Pennsylvania, on the subject of an asylum for the insane poor," written in the author's usual style of excellence, when his sympathies are enlisted in a good cause. It is a statistical paper, of value for future reference, and should therefore be carefully preserved. Owing to the pecuniary difficulties in which that State is now unhappily involved, Gov. Porter could not sanction, conscientiously, a bill for the establishment of an asylum for the insane poor, notwithstanding it passed, last year, both branches of the Legislature. Such perseverance, however, as the friends of humanity manifest in this and the preceding report, will soon bring a State asylum into existence, whether there is money in the treasury or not.

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*Medical Society of Tennessee.*—Minutes of the transactions of the Society, at the eleventh annual meeting, at Nashville, on the 4th of May last, came on by mail last week, somewhat shattered by the journey. Whether it has been six months or six weeks on the way, cannot be readily determined, nor is it of consequence to know—but we are certain of one fact, viz., that it is a day after the fair. The time has passed when it would have been exceedingly valuable to the compilers of medical statistics. Dr. S. Hogg, of Nashville, is president. Other matters relating to the Society, which is characterized by uncommon spirit, will be looked into another day.

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*Physiological Lectures.*—Dr. Reynell Coates, of Philadelphia, who has long been before the scientific public, and consequently extensively known, commenced a course of popular lectures on physiology, in this city, on Monday evening last. A syllabus was published last season, which shows that the subjects embraced in the scheme of his lectures, are both extensive and instructive. The topics discussed are intensely interesting, even to those who possess a systematic acquaintance with physiology, and therefore cannot be less so to those who are for the first time presented with the curious details of an accomplished teacher.

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*Tincture of Muriate of Iron in Diabetes Mellitus.*—Mary Wild, *stat.* 56, of Ashton, had been subject to a diabetic discharge for eight months;

her general health had for some time been very precarious from the cessation of the menstrual discharge. About seven pounds and a half of urine in 24 hours. In this case the saccharine matter was not so abundant as in former cases. I gave the tinct. fer. mur. mixt. for six days, when a slight abatement was observable; but, on the 12th day, the quantity was more than at the commencement. On the 16th, the abatement again showed itself; and from this time to the end of four weeks kept constantly decreasing. At this time pleuritic symptoms called for a cessation of these remedies and the substitution of others, during which time a slight increase of urine came on; but on going on with the old medicine the improvement returned. She finally ceased taking medicine at the end of eight weeks, feeling her health quite restored, and has had no return since. The date of this case was March, 1840.—*Lancet*.

*Statistics of Amputation.*—Dr. Lawrie read to the British Association for the Advancement of Science, a most elaborate paper "on the results of amputation," in which the results of all the cases which had occurred in the Glasgow Infirmary for many years, were stated, and all the great questions connected with amputation were ably discussed in connection with those results which were drawn from tables in which the operations were classified according to the sex of the patients; the cause of the operation, whether disease or accident; the limb operated on, whether primary or secondary, &c. &c. The following are some of the results obtained. Out of 276 cases, 176 recovered, 100 died; 216 were males, 60 females; 153 were for previously existing disease, 123 for injuries. One case of operation at the hip-joint occurred, which recovered. Generally speaking, secondary amputations were preferred, and immediate dressings approved of. The causes of death in the fatal cases were classified, and were frequently found to depend on visceral disease.

From the importance of the subject and the value of the communication, the section recommended its immediate publication *in extenso*.—*Ibid*.

*Four Children at a Birth.*—Jane Boucaut, aged 24 years, married 23 months, was prematurely taken in labor, on the 21st of April, 1831, at six o'clock in the evening, at the seventh month of pregnancy, and was delivered of a male child; twelve hours thereafter she gave birth to a second, then to a third, and immediately afterwards, to a fourth child, all of the male sex. The second birth was accompanied by a fresh discharge of liquor amnii. The two first and the last of the children were equal in size, and as large as usual for fœtuses of the seventh month. The third was smaller, about equal to the fœtus of the fifth month. The latter lived for some seconds, the three larger children for several hours.

There were two placentæ; one of these, attached to the upper of the right side of the uterus, had three cords; the other placenta connected with the uterus in the same situation, on the opposite side, had only one. The single placenta separated easily from the uterus; the other was firmly attached, and required to be removed by the hand. The patient was seriously ill for some time after her confinement, but, at the end of six weeks, was entirely recovered.—*Gaz. Med. de Paris*.

*Excision of the Eye.*—Many very serious accidents have been brought into the Cheltenham General Hospital lately, occurring on the railroads

in progress in this neighborhood. A curious case was admitted on the first of May.

James Mathews, *etat.* 27, was brought in with the globe of the eye completely protruded from the orbit. The accident happened from his head being jammed between two carriages running on the rail road, from one of which an iron plate projected. The eye was removed by dividing the muscles and optic nerve. The nerve was divided at the point where it pierces the sclerotica; the globe was removed quite entire, and the patient did well.—*Lancet.*

TO CORRESPONDENTS.—The communication of "Antiquus" is received. To make room for this and other papers we shall probably omit one of Dr. Paine's Nos. next week, or the week after.

MARRIED.—In Boston, Dr. Edward William Holmes, of Charleston, S. C., to Miss Mary Elizabeth Thacher.

DIED.—At Danvers, Mass., Dr. Edward Southwick, of Augusta, Me., 42.

AGENTS FOR THE MEDICAL JOURNAL.—The safest and generally the most convenient way of forwarding subscriptions for the Journal, is through the mail, by postmasters, who are authorized to frank money letters from subscribers to publishers. In cases where it is preferable, however, subscriptions may be paid to either of the following agents:—

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Number of deaths in Boston for the week ending Nov. 14, 34.—Males, 19—females, 17. Stillborn, 1. Of consumption, 5—old age, 5—typhous fever, 3—erysipelas, 2—suicide, 1—dropsy on the brain, 2—burn, 1—diarrhea, 1—Sis, 4—infantile, 3—decay of nature, 1—lung fever, 2—fever, 1—dysentery, 1—teething, 1—palsy, 1—casualty, 1.

# VERMONT ACADEMY OF MEDICINE.

LECTURES will commence on the second Thursday of March, 1841, and continue fourteen weeks.

Theory and Practice of Medicine, by	- - -	HORACE GREEN, M.D., N. Y.
Anatomy and Physiology, by	- - -	ROBERT NELSON, M.D., N. Y.
Chemistry and Pharmacy, by	- - -	JAMES HADLEY, M.D., Fairfield, N. Y.
Surgery and Medical Jurisprudence, by	- - -	JAMES BRYAN, M.D., Philadelphia.
Materia Medica and Obstetrics, by	- - -	JOSEPH PERKINS, M.D., Castleton, Vt.
General Pathology, by	- - -	C. L. MITCHEL, M.D., N. Y.
Demonstrator of Anatomy	- - -	ROBERT JANISON, M.D., Castleton, Vt.
Castleton, Vt., Nov., 1840.		JOSEPH PERKINS, Registrar.
	N. 18.—IamM&cover	

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N. 11.—



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We, the undersigned, would cordially recommend Mr. C. A. Zeitz as a thorough artist. The surgical instruments of his make, which we have ourselves used, have fully answered our expectations; and we can, therefore, with the more confidence recommend him to the medical profession generally.

JOHN C. WARREN, M.D.

GEO. HAYWARD, M.D.

S. D. TOWNSEND, M.D.

} Surgeons to Mass. Gen. Hospital.

## MEDICAL TUITION FOR 1840-41.

THE subscribers will commence their course of instruction for the ensuing medical year, on November 1st, 1840 (the period at which the Lectures at the Medical College of Harvard University begin).

Minute examinations will be held on all the branches of medicine and surgery during the lectures, in order that students intending to offer themselves for examination at the College in the spring, may be prepared. Students may be assured that they will have constant and abundant opportunities for the cultivation of practical anatomy at all seasons of the year. After the lectures, the arrangements will be as follows until the ensuing November.

Free access at all hours to the United States Marine Hospital at Chelsea will be granted; a daily morning visit will be made by Dr. Stedman, and every week Drs. Perry, Bowditch and Wiley will visit in the afternoon, for the purpose, chiefly, of learning the physical signs of diseases of the chest. Dr. Bowditch will deliver a course of lectures on diseases of the chest and air passages. Admission to the medical and surgical practice at the Massachusetts General Hospital, the Infirmary for Diseases of the Lungs, and to the practice of one of the Dispensary Districts; occasional opportunities for operative surgery and midwifery.

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Boston, August 19, 1840.

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THE subscribers have made arrangements for the treatment of patients suffering from chronic diseases, whereby they can avail themselves of the powerful auxiliary afforded by the use of the Lebanon Spring water, in the form of cold, warm, vapor and shower bath. The Lebanon water, in purity and temperature, has a strong resemblance to the famous Bristol and Buxton waters, and its remedial power is well attested.

August, 1840.

A. 26.—12

JOSEPH BATES, Lebanon Springs.

CHILDS &amp; LEE, Pittsfield.

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*Wm. Thane*